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# Breaking the Crystal Methamphetamine Economy: Illegal Drugs, Supply-side Interventions and Crime Responses

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This paper evaluates the effects on crime of supply-side interventions that restricted access to pseudoephedrine-based medications in the USA, drastically reducing the domestic production of methamphetamine. I find that these government interventions increased property and violent crime by around 3–4%, with criminogenic effects lasting for up to 7 months. Stronger evidence is detected in counties where laboratories producing methamphetamine were previously in operation. My findings suggest that policy interventions that have a limited effect on supply and no impact on the demand for drugs could open up the way to unwarranted crime responses. Timely policy implications are discussed.

## INTRODUCTION

The market for illegal drugs generates an annual cost of \$200 billion in the USA (ONDCP 2010). This figure reflects lost productivity, healthcare expenditures, and—the focus of this paper—property and violent crime. The scale of the drug–crime nexus is dramatic: more than 60% of the current prison population, 1.2 million people, tested positive at the time of arrest for one or more illicit substances. This evidence is even more emblematic considering that less than 9% of the general US population consumes illicit drugs, which suggests that drug users are overly represented behind bars (Mumola and Karberg 2006). Moreover, drug-related crimes are not always directly connected to the consumption of addictive substances, but can also be related to the production and distribution of the drug itself (DEA 2010).

To eradicate these clandestine markets and deter drug usage, the US government widely employs supply-side interventions that target the producers and distributors of illicit substances. Arguably, improving our understanding of the consequences of these government interventions is critical for the design of cost-effective regulatory frameworks: as a policy principle, these should be aimed at reducing the growth of illegal drug markets while also mitigating the risks associated with the emergence of unintended criminal responses.

My paper focuses on methamphetamine, also known as ‘meth’, an addictive neurotoxic stimulant that generates irreversible health damage (Rawson *et al.* 2001; Simon *et al.* 2001; Lynch *et al.* 2003). Methamphetamine is considered one of the most dangerous illegal drugs in the USA because of its alleged effects in fostering both property and violent crime (DEA 2010).

Specifically, I examine the effects on property and violent crime of supply-side policies that have restricted access to pseudoephedrine-based medications. These common medications are a critical input that can be used to synthesize methamphetamine in low-capacity domestic laboratories (labs), which are predominantly operated throughout the USA by local independent methamphetamine users (Eck and Gersh 2000; Sexton *et al.* 2006). In the attempt to constrain the spread of these small labs, in the USA a comprehensive set of state and federal laws regulating the sale of pseudoephedrine-based medications was phased in between 2004 and 2006. These laws targeted such medications by (i) dictating purchasing quotas, (ii) regulating the

placement of the product in stores to reduce the likelihood of theft, (iii) requiring that purchasers be asked to present a form of identification, and (iv) requesting that retailers maintain a logbook of clients to prevent repeated purchases.

Recent work by Dobkin *et al.* (2014) shows that these policies were successful in reducing the number of operating meth labs by around 36%. In particular, the laws heavily disrupted the drug market composed of small-scale illegal operations, which were producing less than two ounces of product at a time. Labs producing larger quantities of methamphetamine were less affected, presumably due to the capacity of professional drug networks to divert larger quantities of pseudoephedrine from other sources. After further investigating the impact of the over-the-counter (OTC) restrictions on drug-related crimes, Dobkin *et al.* (2014) find no evidence of changes in possession and sale of narcotics and non-narcotics. This evidence, alongside the lack of any significant effects on drug consumption and purity-adjusted market prices, suggests that methamphetamine users were able to circumvent the laws, finding the illegal drug elsewhere or through other channels.<sup>1</sup>

I extend the work of Dobkin *et al.* (2014) by asking the following question: does the disruption of crystal methamphetamine domestic markets—through the implementation of OTC restrictions—affect property and violent crime rates? Understanding possible effects of these interventions on property and violent crime is important for at least two reasons. First, this evidence is needed to provide a comprehensive evaluation of the policy, given the close connection existing between criminal activity and the disrupted methamphetamine market. Second, this evaluation can inform policymakers about possible effects on crime of similar interventions aimed at further restricting the access to methamphetamine's chemical precursors. This is an important aspect given that the methamphetamine 'epidemic' is far from over in the USA and many other countries.<sup>2</sup>

The laws dramatically reduced the accessibility of pseudoephedrine-based medication—increasing their price in the underground market (Sexton *et al.* 2006)—suddenly diminishing the availability of methamphetamine produced domestically and the number of operating labs of small dimensions. The laws did not affect larger producers and distributors of methamphetamine and did not change the consumption of the drug. The possible effects on crime are not obvious *ex ante*. On the one hand, the laws might have led to an increase in violence committed by criminal groups fighting over the newly available drug markets. Also, the OTC restrictions might have increased acquisitive crimes due to users' need to compensate for the higher costs of domestic production imposed by the laws, as well as to pay for the resulting increase in the cost of addiction.<sup>3</sup> On the other hand, the OTC restrictions might have reduced crime if a large share of illicit activity was directly originating from the functioning of the domestic production of methamphetamine, and if its sudden reduction allowed police officers to be more effective in deterring other forms of crime. I believe that these ambiguous theoretical predictions raise the need for compelling empirical evidence.

This paper provides credible estimates of the *net* effects of the OTC restrictions on crime. My state-level difference-in-differences analysis, which exploits monthly variation in the staggered implementation of the laws across the USA, indicates that the implementation of the OTC reforms led to criminogenic effects. I find an increase of around 3–4% in both property and violent crimes (larceny and burglary, and assault, respectively). I further examine the local impacts of the regulations matching Federal Bureau of Investigation (FBI) crime data aggregated at the county level to Drug Enforcement Agency (DEA) information on the locations of clandestine meth labs. This allows me to conduct a more flexible investigation of the heterogeneous criminal

responses in counties arguably characterized by a diverse prevalence of methamphetamine production and usage. I find stronger evidence of crime responses in counties where enforcement agencies indicate that meth labs were in operation prior to the implementation of the policy. These results appear to be consistent with the underlying nature of the domestic market disrupted by the laws, in which production, distribution and usage are strongly interconnected and are inherently characterized by a very local dimension (DEA 2010).

An event-study analysis reveals that the effects on crime are well timed, appearing right on impact of the implementation of the policy, and lasting for about 5–7 months. State-level and county-level estimates hold up to an extensive battery of checks, such as the use of linear specifications and count data of criminal episodes. I show that estimated coefficients are stable to the exclusion of covariates and state-specific linear time trends from the estimating equations. Similarly, estimates are robust to the use of state-specific quadratic and cubic trends. This mitigates concerns that non-linear unobservable trends, correlated with a state's decision to implement a law and the propagation of criminal activity within its territory, might be critical confounders in the analysis. Finally, more than 20 placebo tests lend further support to the credibility of the estimates, indicating that my findings on the criminogenic effects of the OTC reforms are indeed reliable.

My findings complement the evaluation of the OTC restrictions conducted by Dobkin *et al.* (2014) by revealing that the policies led to criminogenic effects. It is worth noting that a moderate increase in crime of around 2–4% could hide a more sizeable impact on the criminal propensity of the subpopulation that was most affected by the reforms. At the time of implementation, the approximate share of meth-related crimes in counties where the meth epidemic was occurring was around 20–50% (Kyle and Hansell 2005).<sup>4</sup> Thus my estimates on property crimes—likely committed by a population gravitating around the domestic production of the drug—suggest that the laws might have increased their criminal propensity by 6–20%. Moreover, the economic costs connected with the policies were not negligible. My results, combined with the estimates of the costs of property and violent crimes reported by Heaton (2010), quantify an average economic cost of around \$3.9 million in counties where crime increased as a consequence of the reforms.<sup>5</sup> Overall, my estimates suggest that government interventions that effectively disrupt only part of the illegal supply and leave unchanged the demand for illicit substances can produce unintended crime responses. For these reasons, my results provide direct support for supply-side interventions that should be implemented in conjunction with monitoring programmes and crime-prevention strategies targeting areas characterized by severe drug problems.

My work contributes to the literature investigating the effects of supply-side interventions on criminal activity. The findings are rather mixed. Dobkin and Nicosia (2009) examine the impacts in California of a different supply-side measure targeting methamphetamine in 1995. While they detect a large disruption on both the supply and the demand side of the methamphetamine market, they find no substantial evidence of effects on property and violent crime, with some evidence of a possible increase in robberies. However, the authors argue that these results need to be taken with some caution because the crime responses were concentrated in counties with low methamphetamine usage. In a different context, Adda *et al.* (2014) show that a cannabis depenalization policy in the London borough of Lambeth caused police to reallocate efforts toward non-drug-related crime, leading to a reduction in all such felonies. Dell (2015) uses a regression discontinuity design to show that drug-related violence tends to increase substantially after close elections of National Action Party mayors in Mexico.

Her findings indicate that this violence is caused by rival traffickers' attempts to usurp territories after police crackdowns weaken incumbent criminals. Gavrilova *et al.* (2018) study the effect of medical marijuana laws on crime, and indicate that decriminalization of the production and distribution of drugs could lead to a reduction in violence in markets where organized crime is pushed out by licit competition.<sup>6</sup>

More broadly, my study is connected with the literature examining the drug–crime nexus. Corman and Mocan (2000) provide evidence on the crime-deterrence relationship, using time series variation of a monthly-level dataset for New York City. They find that the effects of law enforcement on crime are stronger and more significant than those of drug usage. Grogger and Willis (2000) study how the emergence of crack cocaine affected crime in 27 US metropolitan areas, and show that the introduction of crack had substantial effects on violent crime but essentially no effect on property crime, suggesting that this technological innovation increased violence on the part of distributors. Angrist and Kugler (2008) study the consequences of a major shift in the production of coca paste from Peru and Bolivia to Colombia, and suggest that the rural areas that saw an increase in coca production became much more violent. Fryer *et al.* (2013) also investigate the impact of crack cocaine on crime, constructing an index based on a range of indirect proxies. Their analysis implies that the greatest social costs of crack have been associated with prohibition-related violence rather than drug use *per se*.

The paper proceeds as follows. Section I discusses the institutional background. Section II describes the data and research design. Section III presents the results. Section IV provides the conclusion and discusses the timely policy implications of my work.

## I. INSTITUTIONAL BACKGROUND

During the period of analysis, methamphetamine was primarily distributed in the USA by professional criminal organizations. These organizations were producing large quantities of methamphetamine in 'super labs' based in Southern California and Northern Mexico. The criminal groups were diverting sizeable quantities of ephedrine or pseudoephedrine—the essential ingredient in the synthesis of crystal methamphetamine—maintaining large-scale operations. Furthermore, they were controlling all aspects related to the distribution and sale of the illegal substance, which was typically distributed in the USA by Mexican drug trafficking groups and US gangs related to them (Gonzales *et al.* 2010).

During the 1990s, small labs producing methamphetamine became widespread in the USA. This growth was fostered by the availability of pseudoephedrine, which was contained in common medicines alleviating flu symptoms. This chemical component could be extracted from these medicines via a simple and inexpensive chemical process that involved little equipment, few supplies, and almost no expertise in chemistry.<sup>7</sup> The domestic producer of methamphetamine was often a heavy user who started manufacturing the illicit drug to sustain his drug addiction. The network gravitating around the domestic production of methamphetamine was characterized by many separate small groups with weak organizational structure, and the production and consumption of the drug happened inside the boundaries of these segmented markets. Methamphetamine produced domestically was typically sold within this close network of drug addicts rather than to strangers on the street (Eck and Gersh 2000).

Small labs were responsible for a minor part of the methamphetamine distributed in the USA. However, they created numerous problems for local communities. The



manufacturing process was highly volatile. Flammable chemicals used in the manufacturing process often resulted in toxic fumes, fires and explosions. These generated serious concerns regarding environmental contamination and dangers to first responders, law enforcement, and other members of society exposed to such labs (Shukla *et al.* 2012). The spread of these labs also led to increased social costs associated with burden of addiction, premature death of users, drug treatment expenditures, lost productivity, healthcare, child endangerment, crime and criminal justice expenses (Nicosia *et al.* 2009).

To reduce the growth of the domestic market for methamphetamine and the various costs associated with its presence, between 2004 and 2005, 35 states passed laws that imposed restrictions on the OTC sale of pseudoephedrine-based medications. The laws targeted pseudoephedrine-based medications by (i) dictating purchasing quotas, (ii) regulating the placement of the product in stores to reduce the likelihood of theft, (iii) requiring that purchasers be asked to present a form of identification, and (iv) requesting that retailers maintain a logbook of clients to prevent repeated purchases. The most common OTC regulations adopted at the state level involved purchasing quotas and required that the pseudoephedrine-based medications were placed in secure locations. The majority of states also required clients to present a form of identification, and 24 states required that retailers maintain a logbook to avoid repeated purchases. In 2006, all jurisdictions in the USA became subject to the Combat Methamphetamine Epidemic Act (CMEA), a federal policy that set a national standard for the legal sale of these medications. In April 2006, purchase limits were applied at the federal level. In September 2006, the other three types of restrictions were implemented. In practice, the stricter provisions were applied in each state, irrespective of whether they were contained in a state law or the federal one. If, for instance, a state did not impose any identification request in its regulation, then starting from September 2006, it had to comply with the identification request contained in the federal act.<sup>8</sup>

Dobkin *et al.* (2014) record the date on which each of these four restrictions went into effect in all states. As in their work, my empirical analysis focuses on the date of first implementation of any of these regulations (I code the implementation of the federal law in April of 2006). Tables A1 and A2 of the Online Appendix document the date of adoption of the various OTC regulations by state. Figure 1 shows the change in the share of states with an applicable regulation, providing an accurate graphical representation of the variation associated with the rollout of the laws, which I exploit in my empirical design. Importantly, the staggered rollout of the policies allows for identifying the effects of the restrictions on crime in each state separately while controlling for common time trends across the USA.<sup>9</sup>

## II. DATA AND RESEARCH DESIGN

### *Data*

The main dataset used in the analysis is the FBI's Uniform Crime Reports (UCR): Offenses Known and Clearances by Arrest. Law enforcement agencies voluntarily report the monthly counts of different types of crime committed in their territory, including murder, rape, assault, robbery, burglary, larceny and motor vehicle theft. The UCR data are the best public available data on crime in the USA; however, not all FBI agencies report crimes each year, and within a given year, not all agencies report crimes in each calendar month.<sup>10</sup> To reduce the concerns related to the entry and exit of law

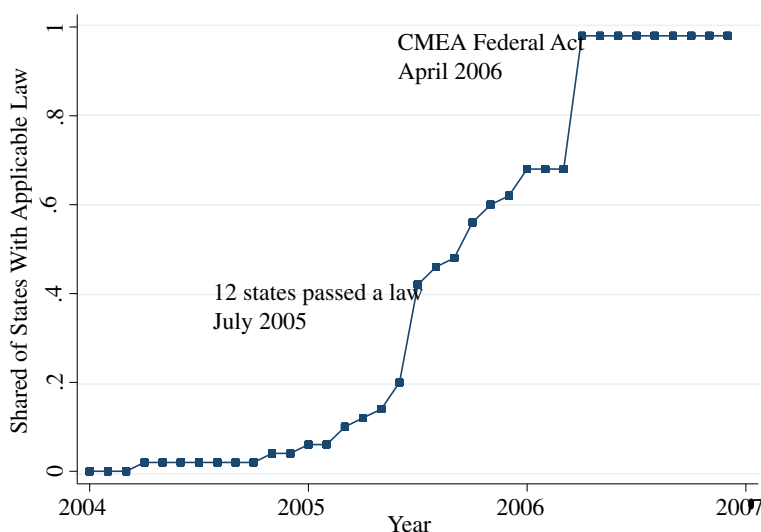


FIGURE 1. Share of US states with applicable OTC restriction.

*Notes:* This figure shows the change in the share of states with an applicable regulation on methamphetamine chemical precursors. It shows that 12 states enacted a law restricting access to pseudoephedrine-based medications in July 2005, and 15 states had a first applicable law (national or federal) in April 2006. All the dates of implementation are reported in Tables A1 and A2 of the Online Appendix (Dobkin *et al.* 2014).

enforcement agencies in the data, my state- and county-level analyses use information extracted from a balanced panel of law enforcement agencies that consistently reported monthly crimes around the time of the implementation of the laws. As in the work of Dobkin *et al.* (2014), I assembled a dataset that starts in January 2002 and ends in March 2008, including 75 month–year periods. This contains monthly information for 48 US states and the District of Columbia (DC), covering 1942 counties. Florida and Rhode Island are excluded from the analysis because FBI agencies within these states did not consistently report crime during the period of interest. The UCR data also contain information about the population covered by each FBI agency. This information allows for the construction of population weights that I employ in the empirical analysis.<sup>11</sup>

The National Clandestine Laboratory Register contains dates and addresses of the locations at which law enforcement agencies reported finding a meth lab.<sup>12</sup> A lab seizure is defined as seizure from ‘an illicit operation consisting of a sufficient combination of apparatus and chemicals that either has been or could be used in the manufacture or synthesis of controlled substances’. The register also records the presence of chemicals or equipment usually associated with the manufacturing of the drug, and the locations at which empty pseudoephedrine containers were abandoned. This information is publicly available from 2004 and is used to identify counties where meth labs were likely to be in operation prior to the implementation of the OTC restrictions. Arguably, in these locations, one can expect to identify larger or more precise effects of the reforms on crime.

The database is further supplemented with state–month measures of unemployment, number of households receiving food stamps, and average temperature and precipitation.<sup>13</sup> Data on weather conditions are unavailable for DC and Hawaii. The main estimating sample includes observations where crime and state-level covariates are

always observed (47 US states in 75 time periods), which allows me to capture changes in coefficients across specifications that are not driven by changes in the composition of the sample. However, I show in the Online Appendix estimates obtained sequentially, including state-specific trends and covariates. Table 1 provides descriptive statistics on crime and state covariates that are not discussed due to brevity considerations only. Descriptive statistics on meth labs will be discussed when relevant for the empirical analysis.

### Research design

*State-level analysis* I use the following model to estimate the effects of the OTC regulations on crime:

$$(1) \quad y_{s,t} = \beta_1 OTC_{s,t} + \alpha_s + \gamma_t + \mu_s \times t + \beta_2 X'_{s,t} + \epsilon_{s,t},$$

where  $y_{s,t}$  is the crime outcome in state  $s$  at time  $t$  (month–year level, e.g. April 2005). The variable of interest,  $OTC_{s,t}$ , is an indicator set to 0 in the months prior to the implementation of a state's regulation, and it takes the value 1 in the months afterwards. I set this variable as the fraction of the month in which the regulation was active during the month when a regulation was first implemented.<sup>14</sup> State fixed effects  $\alpha_s$  absorb

TABLE 1  
DESCRIPTIVE STATISTICS

	N	Mean	S.D.	Min	Max
<i>Property crimes</i>					
Larceny	3675	210.7	54.2	75.4	547.6
Burglary	3675	67	21.6	14.5	125.4
Motor vehicle theft	3675	42.2	19.1	1.5	184.8
<i>Violent crimes</i>					
Robbery	3675	14.8	7.2	0	84.6
Assault	3675	122.7	44.8	31.8	372.2
Rape	3675	2.8	1.1	0	29.5
Murder	3675	0.6	0.3	0	6
<i>Controls</i>					
Unemployment	3675	5	1.1	2.3	9.5
Food stamps	3675	204,025	208,391	4481	1,156,940
Average precipitation (mm)	3525	3.1	2	0	15.9
Average temperature (°F)	3525	51.3	17.8	-4.7	84.4

### Notes

Descriptive statistics at the state–month level from January 2002 to March 2008. Data are for 48 US states plus the District of Columbia (DC). Florida and Rhode Island are not included in the analysis because FBI agencies in these states did not consistently report crime data during the period under analysis. The panel includes 75 months (49 states \* 75 months = 3675 observations). Data on weather conditions are unavailable for DC and Hawaii (3675 – (75\*2) = 3525). Descriptive statistics on crime are expressed per 100,000 people and are weighted by population as in estimating equation (1). Precipitation is expressed in millimetres. Temperature is expressed in degrees Fahrenheit. Data sources: crime, FBI's Uniform Crime Reports—Offenses Known and Clearances by Arrest; unemployment, US Census Bureau; number of households receiving food stamps, Supplemental Nutrition Assistance Program, US Department of Agriculture; weather, US National Climatic Data Center.



unobservable time-invariant differences across states. Month–year fixed effects  $\gamma_t$  control for uniform changes in criminal activity in the USA, fitting a different intercept for each of the 75 periods in the sample (e.g. intercept for May 2005, intercept for June 2005). State-specific linear trends  $\mu_s \times t$  capture potential unobservable confounders related to both the implementation of the OTC laws in a state and the propagation of criminal activity in its territory. I also add a vector of state–month covariates  $X'_{s,t}$ , as discussed above. Standard errors are clustered at the state level. The coefficient of interest,  $\beta_1$ , measures the effect of the OTC regulations on crime. In the baseline specification, crime is log transformed using  $\log(1+z)$ , where  $z$  is the crime rate per 100,000 people. However, I will show very similar results when I use a linear specification that includes crime rates as outcome or a Poisson model that uses the count of criminal episodes.

My analysis uses data assembled from the population of FBI agencies consistently reporting crime during each month between January 2002 and March 2008. Given that the population of the FBI agencies reporting crime varies widely across states and counties, I employ a weighted least squares (WLS) estimator, using as weight the population covered by the FBI agencies reporting crime in a state–year. The WLS estimator consistently estimates the population linear projection of the dependent variable on the explanatory variable; that is, it estimates the impact of the OTC restrictions on crime rates experienced by the population in the sample that was affected by the regulation. Also, given that in my data the population varies widely across geographical groups, the group average error term is heteroscedastic, and the ordinary least squares (OLS) estimation is inefficient and leads to inconsistent standard errors. Instead, the WLS estimator is the minimum variance linear unbiased estimator that produces consistent standard errors correcting for heteroscedasticity (Deaton 1997; Solon *et al.* 2015). In the robustness section, I will compare size and precision of the point estimates using the WLS and OLS estimators. The latter applies the same weights to all observations in the sample, regardless of the population included in each geographical cluster.

*Event-study analysis* I further investigate the consistency of the results by implementing an event-study analysis, which enhances our understanding of the impacts of the OTC reforms on crime in at least two ways. First, it allows for examining the presence of possible ‘pre-trends’ that might confound the estimates of the coefficient of interest,  $\beta_1$ . Second, it helps to identify the dynamic effects of the regulations, whether the crime responses emerge on impact of the enactment of the laws, and their persistence over time. I estimate the equation

$$(2) \quad y_{s,t} = \sum_{i=-12}^{12} \mu_i 1(\rho_{s,t} = i) + \alpha_s + \gamma_t + \mu_s \times t + \beta_3 X'_{s,t} + \epsilon,$$

where the indicator  $\rho_{s,t}$  measures the month relative to the introduction of an OTC regulation. I define  $\rho_{s,t} = 0$  if state  $s$  enacted an OTC regulation at any time during month  $t$ . For each specification, I estimate 25  $\mu_i$  coefficients associated with indicator variables running from 12 months before the implementation of the laws to 12 months after.<sup>15</sup> All other details are equal to estimating equation (1): Crime is log-transformed; I include state fixed effects, time fixed effects, state-specific linear trends and state–month covariates; regressions are weighted by the population covered by the FBI agencies reporting crime in a state–year; and standard errors are clustered at the state level.

*County-level analysis* The baseline analysis exploits state-level variation that originates from the different timing of implementation of the laws across various US states. However, given that the intensity of the methamphetamine epidemic might significantly vary within a state, I also explore the county-level dimension of the data. Because the FBI agency-level data can be aggregated at the county level, and the National Clandestine Laboratory Register contains the addresses of the locations at which law enforcement agencies indicated the presence of clandestine drug labs, I employ a more flexible empirical strategy to examine the presence of heterogeneous criminal responses in counties arguably characterized by a larger domestic production of methamphetamine. Specifically, I compare the impact of the reforms in ‘Meth producer’ counties (i.e. counties where at least one lab was reported to be in operation prior to the implementation of the laws) to ‘Other’ counties (where no such labs were found). I estimate the model

$$(3) \quad y_{c,t} = \beta_4 OTC_{s,t} + \alpha_c + \gamma_t + \mu_s \times t + \beta_5 X'_{s,t} + \epsilon_{c,t},$$

where crime  $y_{c,t}$  is measured in a county  $c$  at month–year  $t$ . The assembled county-level panel dataset allows for the inclusion of county fixed effects  $\alpha_c$  that control for unobservable time-invariant differences across counties. The estimating equation also includes time-fixed effect  $\gamma_t$ , state-specific linear trends  $\mu_s \times t$ , and state–month covariates  $X'_{s,t}$ .<sup>16</sup> The coefficient of interest,  $\beta_4$ , identifies within-county changes in criminal activity due to the implementation of the OTC reforms. Crime is log-transformed as in estimating equation (1), but very similar results will be shown when using crime rates or count data of crime as outcome variables. Regressions are weighted by the population covered by the FBI agencies reporting crime in a county–year. Given that, as in equation (1), I use state-level variation associated with the implementation of the policy, I cluster standard errors at the state level.

### III. RESULTS

#### *The effects of the OTC restrictions on crime*

*Results* Table 2 reports the  $\beta_1$  coefficients estimated using model (1). Panel A shows the estimates of the effects of the OTC restrictions on larceny, burglary and assault in columns (1), (2) and (3), respectively.<sup>17</sup> I find that the implementation of the OTC reforms led to criminogenic effects: I detect a 3.2% increase in larcenies ( $p < 0.05$ ), a 3% increase in burglaries ( $p < 0.05$ ), and a 2.8% increase in aggravated assaults ( $p < 0.01$ ).

Similarly, panel B of Table 2 shows the effects of the reforms on these crimes, but it excludes from the sample US jurisdictions that did not adopt any internal measure to regulate the sale of pseudoephedrine-based medication, possibly because they were less affected by the domestic production of methamphetamine. These excluded jurisdictions were subject to only the federal CMEA.<sup>18</sup> In practice, I argue that the exclusion from the sample of jurisdictions only marginally affected by the domestic production of methamphetamine should not heavily affect my estimates if the criminogenic effects of the OTC restrictions are mainly related to the disruption of this illegal operation.<sup>19</sup> Consistent with this hypothesis, panel B shows  $\beta_1$  point estimates that range from 3.6% to 2.7%, all precisely estimated with  $p < 0.05$  for burglary and assault, and  $p < 0.01$  for larceny. The stability of the results in this subsample of states provides initial reassurance

TABLE 2  
THE EFFECT OF THE OTC RESTRICTION ON CRIME: STATE-LEVEL ANALYSIS

	Larceny (1)	Burglary (2)	Assault (3)
<i>Panel A: Sample including 47 states</i>			
OTC restriction	0.0323** (0.0130)	0.0302** (0.0129)	0.0279*** (0.00907)
<i>Panel B: Sample excluding states where only federal law applied</i>			
OTC restriction	0.0366*** (0.0142)	0.0322** (0.0138)	0.0272** (0.0120)

*Notes*

This table shows the  $\beta_1$  coefficients on larceny, burglary and assault obtained using equation (1), which includes state and month-year fixed effects, state linear trends, and covariates. The variable of interest, 'OTC restriction', is an indicator set to 0 in the months prior to implementation of a state's regulation, and to 1 afterwards, and the fraction of the month in which the regulation was active during the month when a regulation was first implemented. Panel A shows results for the entire sample of analysis (47 states and 3525 observations). Panel B shows results for a subsample excluding jurisdictions that did not pass a state law and were subject to only the federal one (39 states and 2595 observations). The excluded jurisdictions are Connecticut, DC, Maryland, Massachusetts, Nevada, New Hampshire, New York, Pennsylvania and Utah. Crime is transformed into logarithmic form  $\log(1 + z)$ , where  $z$  is the crime rate per 100,000 people. Regressions are weighted by population of the agencies reporting crime in a state-month. Standard errors clustered at the state level are shown in parentheses.

\*\*\*, \*\*, \* indicate significance at the 1%, 5%, 10% level, respectively.

that the crime responses are closely related to the disruption of the domestic market for methamphetamine, rather than to seemingly unrelated factors.

*Robustness checks* I show the sensitivity of  $\beta_1$  when state-specific linear trends and all the covariates are sequentially included in estimating equation (1) for the entire sample of the analysis (Table A3 of the Online Appendix) and for the sample that excludes states subject to only the federal CMEA (Table A4). Table A3 shows that the estimate for aggravated assault is stable (ranging from 2.9% to 3.1%) and always highly significant ( $p < 0.01$ ) across specifications. The estimates for larceny gain in size and precision when adding state-specific linear trends, from 2.5% ( $p < 0.1$ ) to 3.2% ( $p < 0.05$ ), and are stable when including covariates. Similarly, the estimates for burglary gain in precision and size when state linear trends and covariates are included: 2.1% ( $p < 0.11$ ), 2.5% ( $p < 0.1$ ), and 3% ( $p < 0.05$ ), respectively. This indicates that state-specific linear trends and covariates absorb omitted factors negatively correlated to the implementation of the laws and the propagation of burglary and larceny within a state. This could suggest, for instance, that the implementation of the OTC regulations might be correlated with the participation or the intensity of social programmes (such as community aid or addiction treatment programmes), which—in turn—might have led to a reduction of economically motivated crimes.<sup>20</sup> Table A4 of the Online Appendix, which reduces the noise in the estimates by excluding from the sample states possibly less affected by the domestic production of methamphetamine where only the federal CMEA was implemented, shows highly significant estimates across specifications for larceny and assault (at least  $p < 0.05$  in all specifications). The estimated coefficients for burglary still gain in size and precision when trends and covariates are added to the estimating equation: 2.1% ( $p < 0.13$ ) at the baseline; 2.4% ( $p < 0.1$ ) when state linear trends are added; 3.2% ( $p < 0.05$ ) when covariates are included. Table A5 of the Online Appendix shows a similar exercise in

TABLE 3  
ROBUSTNESS CHECKS: STATE LEVEL ANALYSIS

	Linear model (1)	Poisson count data (2)	Log model (1) (3)	+ State quadratic trends (4)	+ State cubic trends (5)	No implementation month (6)	Balanced panel (7)
<i>Panel A: Larceny</i>							
OTC	7.366***	0.0319***	0.0323**	0.0223*	0.0252**	0.0302**	0.0327**
restriction	(2.842)	(0.0117)	(0.0130)	(0.0123)	(0.0121)	(0.0126)	(0.0145)
<i>Panel B: Burglary</i>							
OTC	1.959**	0.0250**	0.0302**	0.0210*	0.0238*	0.0280**	0.0304*
restriction	(0.903)	(0.0119)	(0.0129)	(0.0125)	(0.0123)	(0.0127)	(0.0159)
<i>Panel C: Assault</i>							
OTC	3.942***	0.0232***	0.0279***	0.0210**	0.0222***	0.0276***	0.0200**
restriction	(1.175)	(0.00676)	(0.00907)	(0.00956)	(0.00769)	(0.00987)	(0.00785)
Observations	3525	3525	3525	3525	3525	3478	2209
Number of states	47	47	47	47	47	47	47

Notes

This table shows robustness checks for estimating equation (1). I use the sample of 47 states. Column (1) shows a linear specification with crime rates per 100,000 people as its outcome. Column (2) shows a Poisson model with crime count data. Column (3) shows a log model (baseline specification as in panel A of Table 2). Column (4) adds state quadratic trends to column (3). Column (5) adds state cubic trends to column (4). Column (6) excludes the month in which the reform was implemented, where the OTC restriction is coded as a fraction of the month when the law was implemented (3525 – 47 = 3478). Column (7) uses a balanced panel of 23 months before and after the month in which the regulation was implemented, so that each state is observed for the exact number of times (47 states \* 47 time periods = 2209 observations). Crime is transformed into logarithmic form  $\log(1 + z)$ , where  $z$  is the crime rate per 100,000 people, unless otherwise specified. Regressions are weighted by population of the agencies reporting crime in a state-month (but not in the Poisson model, as the likelihood function fails to converge when state-specific trends are included). Standard errors clustered at the state level are shown in parentheses.

\*\*\*, \*\*, \* indicate significance at the 1%, 5%, 10% level, respectively.

which trends and covariates are sequentially included, focusing on the counties where meth labs were in operation prior to the enactment of the policy (both in the sample including all states and in the sample that excludes states that only enacted a federal law). Estimates with and without trends and covariates are shown to be precise under the conventional significance levels for all crimes in this subset of counties, where we should expect the laws to have more ‘bite’. In particular, when ‘Meth producer’ counties in states that enacted a state law are considered, the estimates for burglary range from 3.5% to 4% and are always highly significant ( $p < 0.01$ ). Tables A3 and A4 also report results for other crimes and show no large or significant effects of the reforms on motor vehicle thefts, robbery, assault and rape. Thus the remainder of the analysis will exclusively focus on larceny, burglary and aggravated assault.<sup>21</sup> Table 3 reports further robustness checks for model (1) on these crimes (panels A, B and C, respectively).

Column (1) of Table 3 investigates the robustness of the estimates to changes in the functional form, reporting the results of a linear specification using crime rates per 100,000 people as its outcome. I detect point estimates of 7.4 ( $p < 0.01$ ) for larceny, 1.9 ( $p < 0.05$ ) for burglary, and 3.9 ( $p < 0.01$ ) for assault. Considering the mean of these outcome variables reported in Table 1, the linear specification provides results of very similar magnitude and precision compared to the log-transformed baseline estimates. Column (2) shows the estimates obtained via a Poisson model using count data of crime as its outcome. Again, these estimates are very similar in size and precision to the baseline results. For the sake of comparison, column (3) reports the baseline estimates of model (1) obtained using a log-specification that includes state fixed effects, year fixed effects, state-specific linear trends, and covariates. Column (4) adds to column (3) state-specific quadratic trends. Column (5) adds to column (4) state-specific cubic trends. On the one hand, these specifications absorb non-linear unobservable patterns associated with the passage of the reforms and with the propagation of crime within a state. That is, the inclusion of state-specific trends helps to remove potentially spurious correlations that could be wrongly interpreted as caused by the independent variable that moves along a trend correlated with the trend of the outcome. On the other hand, superimposing exogenous trends could remove genuine correlation, over-absorbing the effect of interest, producing overfitting, leading to conservative and imprecise estimates (Buonanno *et al.* 2011). Columns (4) and (5) show that the results are little affected by the inclusion of these non-linear trends; however, the estimates are still positive and significant. When including state quadratic trends, I detect an increase of 2.2% ( $p < 0.1$ ) for larceny, 2.1% ( $p < 0.1$ ) for burglary, and 2.1% ( $p < 0.05$ ) for assault. When including state cubic trends, I detect an increase of 2.5% ( $p < 0.05$ ) for larceny, 2.4% ( $p < 0.1$ ) for burglary, and 2.3% ( $p < 0.01$ ) for assault. Overall, this exercise appears to attenuate the concerns that the estimates of  $\beta_1$  might be driven by state-specific non-linear time trends correlated with both the implementation of the reforms and the propagation of crime within a state. Taken at face value, conservative estimates obtained including up to state-cubic trends, reveal an impact of the laws on crime in the range 2–2.5%.

In column (6) of Table 3, I exclude the month in which the reform was enacted, where the OTC restrictions are coded as a fraction of the month during which the law was operating. Estimates are qualitatively and quantitatively similar to the baseline. Column (7) reports estimates obtained using a balanced panel of 23 months before and after the regulations, where all states are observed for the exact number of periods. Estimates are similar in terms of size and precision for burglary and larceny. For assault, I observe a drop of around 28% in the point estimate (0.02 instead of 0.028, with  $p < 0.05$ ). In this exercise, I use a balanced sample around the time of first implementation of

the reforms. This implies that relatively more observations from states that adopted the laws early in time (and that are therefore tracked for longer in my data) are excluded from this analysis. The smaller estimate may suggest that in some of these states, the effects on assault were relatively large two years after the implementation of the laws. However, given that this specification excludes 40% of the observations, smaller estimates could also be due to random sampling variability.

*Weighted vs. unweighted estimates* In the baseline analysis, I employ a WLS estimator using as weight the population covered by the FBI agencies reporting crime in a state-year. This is because the population of the FBI agencies reporting crime varies widely across states. The WLS estimator consistently estimates the population linear projection of the dependent variable on the explanatory variable, and it produces consistent standard errors correcting for heteroscedasticity (Deaton 1997; Solon *et al.* 2015). In Table A7 of the Online Appendix, I report weighted and unweighted estimates, with the latter applying the same weights to all observations in the sample, regardless of the population included in each geographical cluster. Column (1) shows the weighted baseline, and column (2) reports OLS unweighted estimates. OLS estimates are less precise, with standard errors increasing by 15%, 30% and 50% for larceny, burglary and assault, respectively. In terms of size, estimates for aggravated assault are rather stable across specifications. Estimates for larceny and burglary are smaller when using the OLS estimator. Differences between weighted and unweighted estimates signal the presence of heterogeneous effects by population (Solon *et al.* 2015). These are investigated in the remainder of the table. I split the sample using the median of the state population, and show weighted and unweighted results in each subsample. Estimates indicate that the effects of the policy are stronger in highly populated states. Point estimates for all crimes are in the range 3–4% and are always highly significant. Also, when focusing on the sample above the population median, we observe that weighted and unweighted estimates are similar in terms of magnitude and precision. Estimates for the states below the median are generally imprecise and of smaller magnitude, with the smallest point estimates detected for larceny. Overall, these exercises indicate that the laws restricting the OTC restrictions had a larger and more significant effect on property crimes in highly populated areas. A possible explanation could be that the expected returns from theft could be larger in these areas, as there could be more targets for criminals, thus incentivizing the increase in property crimes.

*Placebo tests* I now present the results of various placebo tests, where the primary analysis implemented using model (1) is replicated with a pseudo treatment variable that—deliberately—does not affect the outcome of interest. The true value of the estimand is zero, and the goal of the placebo analysis is to provide support for the identification strategy behind the primary analysis by assessing whether the pseudo treatment leads to estimates that are close to zero, taking into account the statistical uncertainty (Athey and Imbens 2017).

Table 4 presents the results for larceny, burglary and assault (panels A, B and C, respectively). Column (1) reports the estimates of  $\beta_1$  obtained in the primary analysis for initial comparison. In column (2), the pseudo treatment variable is drawn from the same distribution as the original variable used in the primary analysis, but its values are randomly assigned to the outcome. In column (3), I preserve the real within-state time structure of the laws, randomly assigning crime outcomes to the original treatment variable. Following Neumayer and Plümper (2017), in both columns (2) and (3), I



TABLE 4  
PLACEBO TESTS

	Estimates equation (1) (1)	Placebo tests							
		Random values (2)	Random sequences (3)	4 years before (4)	3 years before (5)	2 years before (6)	2 years after (7)	3 years after (8)	4 years after (9)
<i>Panel A: Larceny</i> OTC restriction	0.0323** (0.0130)	0.00003 (0.00229) 0.419	-0.00102 (0.03624) 0.452	0.0108 (0.0156)	0.0380** (0.0162)	0.0120 (0.0129)	0.00206 (0.0186)	-0.00978 (0.0145)	0.0176 (0.0188)
<i>p</i> -values (1000 permutations)									
<i>Panel B: Burglary</i> OTC restriction	0.0302** (0.0129)	0.00016 (0.00269) 0.458	-0.00148 (0.0503) 0.441	-0.00553 (0.0171)	0.0277 (0.0176)	0.00617 (0.0173)	-0.00235 (0.0173)	0.0161 (0.0161)	0.0243 (0.0185)
<i>p</i> -values (1000 permutations)									
<i>Panel C: Assault</i> OTC restriction	0.0279*** (0.00907)	0.00001 (0.00223) 0.471	-0.00066 (0.0512) 0.456	0.000811 (0.0177)	0.000799 (0.0126)	-0.00968 (0.00963)	-0.00247 (0.0157)	0.0155 (0.00981)	-0.00413 (0.0131)
<i>p</i> -values (1000 permutations)									

Notes

This table shows placebo tests for estimating equation (1). Each panel reports results for a different crime. Column (1) reports estimates of the effects of the OTC restriction as an initial comparison. In column (2) (Random values), the ‘OTC restriction’ variable is drawn from the same distribution as the original variable, but its values are randomly assigned to the outcome. In column (3) (Random sequences), I preserve the real within-state time structure of the laws, randomly assigning crime outcomes to the original variable ‘OTC restriction’. In both columns (2) and (3), I perform 1000 permutations for each crime to minimize the likelihood that a single distributed placebo variable could affect the results by chance. I report the means of the point estimates, the standard errors, and the *p*-values. Columns (4)–(9) show placebo estimates where the ‘OTC restriction’ variable preserves the real within-state time structure of the laws, but it is moved 4, 3 and 2 years before and after the real implementation date. In each of these regressions, the sample is moved accordingly, starting and ending 4, 3 and 2 years earlier and later, respectively (thus the estimating samples always include 75 month-year periods, as shown in column (1)). Columns (4)–(9) do not include state covariates because missing values in past or future periods for these variables would generate samples of different dimensions across placebo specifications estimated in different time frames. Estimating samples from columns (1)–(9) include 47 states and 3525 observations.

\*\*\*, \*\*, \* indicate significance at the 1%, 5%, 10% level, respectively.

perform 1000 permutations for each crime to minimize the likelihood that a single distributed placebo variable could affect the results by chance. I report the mean of the point estimates, the standard errors, and the  $p$ -values. Columns (4)–(9) show other placebo estimates where the pseudo treatment also preserves the within-state time structure of the laws used in the primary analysis, but it is moved 4, 3 and 2 years before and after the real implementation date. In each of these regressions, the sample is moved accordingly, starting and ending 4, 3 and 2 years earlier and later, respectively. This allows for consistency with the primary analysis, which always uses a sample that includes 75 time periods.<sup>22</sup> Out of the 24-point estimates reported in Table 4, the placebo regression reveals a positive and significant coefficient only for larceny, when the enactment date is moved 3 years before the real date of implementation (coefficient 3.8% with  $p < 0.05$ ). The estimates in the remaining 23 placebo exercises are close to zero and are not precisely estimated under the conventional significance levels. The evidence provides further support for the identification strategy behind the primary analysis, indicating that the estimates of the effects of the OTC reforms on crime are indeed reliable.

*Event study* I now examine the dynamics of the intervention in an event-study analysis. Figure 2 reports point estimates of dummy variables indicating the months following (leading up to) the implementation of the OTC restrictions. I use estimating equation (2), which includes state fixed effects, month-year fixed effects, state linear trends, and covariates.

The effects on crime are well timed, appearing right on impact of the implementation of the policy. The magnitude of the effects is moderate and in line with the estimates of model (1): Effects are slightly larger for property than for violent crimes. The impact of the reforms fades out 5 months after the rollout of the laws for larceny and burglary, while it lasts slightly longer for aggravated assault, returning to pre-intervention levels after 7 months. Trends appear to be relatively flat prior to the implementation of the laws. In particular, highly significant estimates with  $p < 0.01$  are detected for larceny during the 5 months after the passage of the law, except in period 1 (the month after the implementation), where  $p < 0.05$ , and period +1, where the estimate is not precise under the conventional significance levels. For burglary, estimates become positive during the month of implementation and are significantly different from zero in periods +2 and +3 ( $p < 0.05$ ) and in period +4 ( $p < 0.01$ ). For assault, significant estimates are detected during the 7 months after the implementation of the laws ( $p < 0.05$ ), except in periods 0 and +3, where estimates are significant at the 10% level. The analysis of the time preceding the implementation of the laws reveals a significant effect in just one out of the 36 pre-intervention dummies (for burglary in period –8, with  $p < 0.1$ ). This seems to reduce the concerns that pre-trends might be a significant driver of the findings.<sup>23</sup>

### *Subgroup analysis*

The county-level data allow for a more flexible investigation of heterogeneous criminal responses in counties arguably characterized by a diverse pre-intervention prevalence of methamphetamine. In this subsection, I investigate whether the laws generated larger crime responses in counties where domestic production was likely taking place. Specifically, I locate these counties using DEA information on meth lab seizures prior to the intervention. Figure 3 shows that the prevalence of methamphetamine production

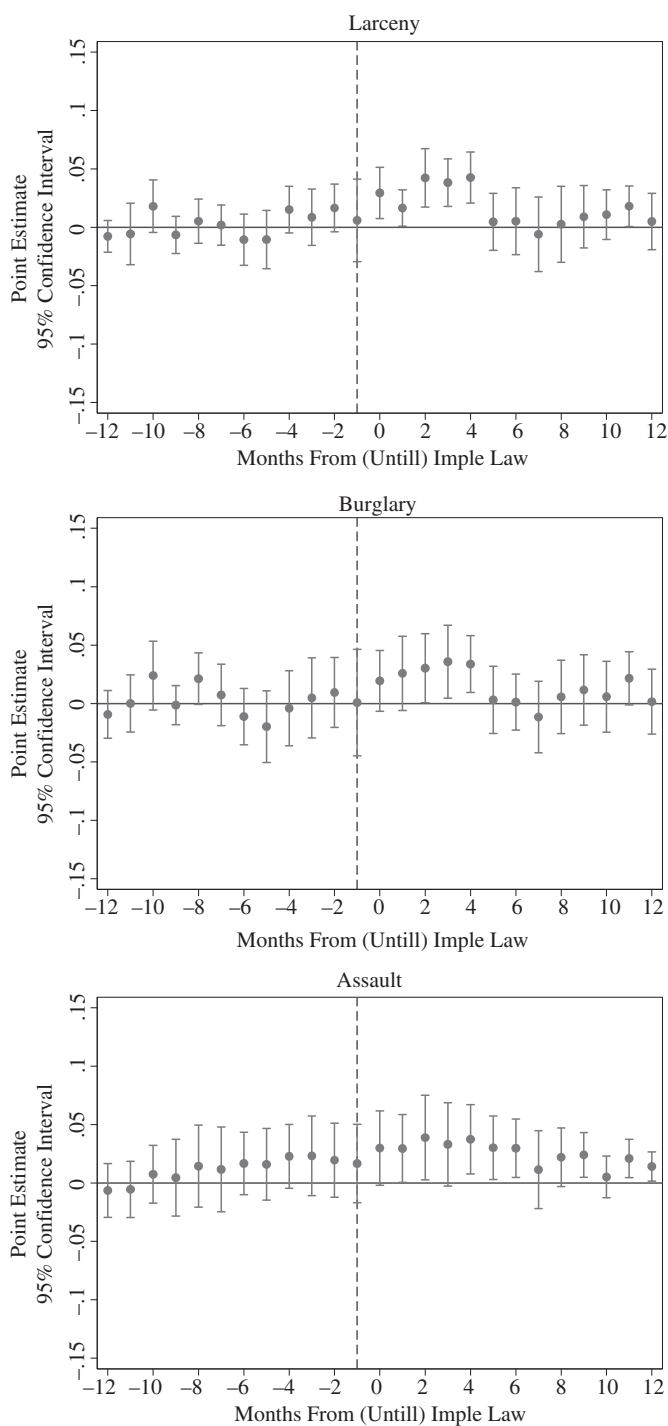


FIGURE 2. Event-study analysis (point estimates and confidence intervals).

*Notes:* This figure plots point estimates and 95% confidence intervals of rollout dummies obtained using equation (2), which includes state fixed effects, month–year fixed effects, state linear trends, and covariates. I show the results for larceny, burglary and assault.

varies widely within the USA. This map shows counties where at least one meth lab was seized in 2004, coloured in dark grey. Counties where no meth lab was seized in 2004 are coloured in light grey. The majority of labs are located on the West coast, in the Midwest, and in the East South-Central regions of the USA.

In Table 5, I report results obtained using model (3) for larceny, burglary and aggravated assault. For each of these crimes, I report  $\beta_4$  coefficients estimated in two separate samples: First, I focus on the sample of 721 ‘Meth producer’ counties where at least one lab was discovered in 2004. I posit that in this subset of counties, the reforms might have had larger effects on crime. Second, I focus on 1219 ‘Other’ counties where no lab was found in 2004. Arguably, in these areas, the domestic production of methamphetamine was less prevalent, and the laws restricting the sale of pseudoephedrine-based medications may have had a minor impact on local crime.

As in the preceding part of the analysis, panel A of Table 5 shows the estimates of the entire sample of states. For ‘Meth producer’ counties, I detect an increase of 3.9% in larceny ( $p < 0.01$ ), 3.3% in burglary ( $p < 0.05$ ), and 4% in assault ( $p < 0.01$ ). As previously hypothesized, these effects are larger in counties where at least one lab was found in 2004. The point estimates for ‘Other’ counties are around one-third of the corresponding estimates for ‘Meth producer’ counties for larceny and assault, and half the size for burglary. Estimates for ‘Meth producer’ counties are not significantly different from zero.

In panel B of Table 5, I repeat the same exercise, but I focus on the restricted sample, which excludes jurisdictions that were subject to only the federal CMEA. Looking at the ‘Meth producer’ counties, the  $\beta_4$  estimates for burglary and larceny are larger than the analogous ones in the sample including all states (4.4% and 4%, respectively). For assault, the estimate is slightly smaller than the corresponding one in panel A (3.6%). These point estimates are precisely estimated at the 1% risk level. Focusing on ‘Other’ counties instead, I detect a positive effect of 2.4% ( $p < 0.1$ ) on larceny. However, some caution is warranted when interpreting this last estimate, as it is highly sensitive to the inclusion of state linear trends and state covariates (see panel D of Table A6 in the

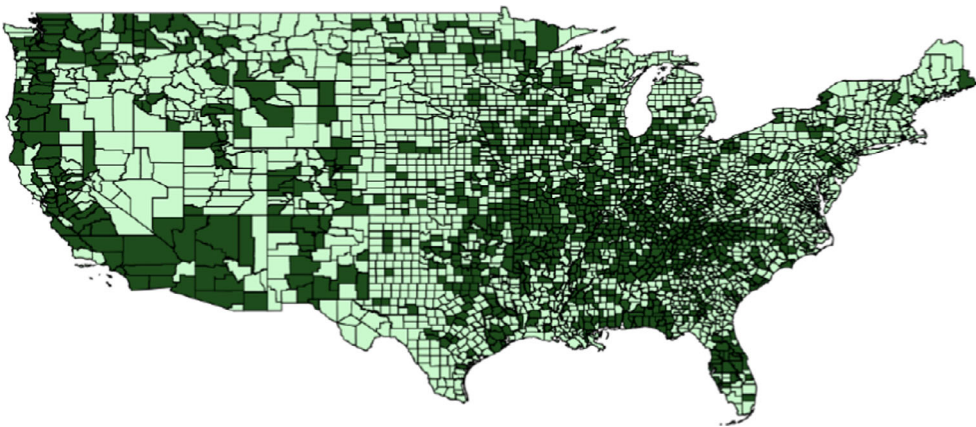


FIGURE 3. Lab seizures pre-intervention (year 2004).

*Notes:* This map shows counties where at least one meth lab was seized in 2004 (‘Meth producers’) coloured in dark grey. Counties where no meth labs were seized in 2004 (‘Others’) are coloured in light grey. Alaska and Hawaii are eliminated from the figure for illustrative purposes only. Source: DEA (2010).

TABLE 5  
THE EFFECT OF THE OTC RESTRICTION ON CRIME: COUNTY-LEVEL ANALYSIS

	Larceny Meth producers (1)	Others (2)	Burglary Meth producers (3)	Others (4)	Assault Meth producers (5)	Others (6)
<i>Panel A: Sample including 47 states</i>						
OTC restriction	0.0393*** (0.0150)	0.0121 (0.0119)	0.0332** (0.0137)	0.0161 (0.0159)	0.0400*** (0.0100)	0.0129 (0.0143)
<i>p</i> -value: Meth producers = Others	0.15		0.41		0.11	
Counties	721	1219	721	1219	721	1219
Outcome mean	232.91	180.92	75.81	54.99	129.46	114.37
Observations	54,075	91,425	54,075	91,425	54,075	91,425
<i>Panel B: Sample excluding states where only federal law applied</i>						
OTC restriction	0.0441*** (0.0158)	0.0239* (0.0135)	0.0403*** (0.0136)	0.0147 (0.0210)	0.0365*** (0.00992)	0.00958 (0.0193)
<i>p</i> -value: Meth producers = Others	0.33		0.28		0.20	
Counties	695	1064	695	1064	695	1064
Outcome mean	235.16	191.61	76.72	60.58	130	120.99
Observations	52,125	79,800	52,125	79,800	52,125	79,800

*Notes*

The table shows the  $\beta_4$  coefficients obtained using model (3) for larceny, burglary and assault. The column 'Meth producers' shows results for counties where at least one methamphetamine lab was seized in 2004. The column 'Others' shows the results for counties where no meth labs were seized in 2004. Panel A uses the sample including 47 states. Panel B eliminates from the sample states that were subject to only the federal law. Jurisdictions excluded are: Connecticut, DC, Maryland, Massachusetts, Nevada, New Hampshire, New York, Pennsylvania and Utah. Crime is transformed into logarithmic form  $\log(1 + z)$ , where  $z$  is the crime rate per 100,000 people (the mean for each subsample is reported in the table). I report *p*-values from hypothesis tests that the coefficients obtained in meth producers and other counties are equal. All regressions include county fixed effects, year-month fixed effects, state linear trends, and covariates. Regressions are weighted by population of the agencies reporting crime in a county-month. Standard errors clustered at the state level are shown in parentheses.

\*\*\*, \*\*, \* indicate significance at the 1%, 5%, 10% level, respectively.

Online Appendix). Estimates in 'Other' counties are around one-third to half the size of the corresponding estimates for 'Meth producer' counties and are not significantly different from zero.

*Discussion* This part of the analysis substantiates the underlying hypothesis of the work: The increase in crime appears to be closely related to the disruption of the local production of methamphetamine.<sup>24</sup> However, two aspects need to be discussed. First, estimates for 'Meth producer' and 'Other' counties, while of different sizes and precisions in the two subsamples, are not significantly different from each other. The *p*-values for the differences of the estimates do not reject the null hypothesis of equality of the coefficients under the conventional significance levels (as reported in Table 5). This could be due to a variety of factors. Given that the seizures represent only a share of the labs effectively in operation, one cannot exclude the presence of labs producing

methamphetamine in ‘Other’ counties. Also, there might be crime spillovers across counties with diverse methamphetamine problems. These possible ‘contaminations’ might reduce the difference in the effects detected across these two subsets of counties. In sum, these results further corroborate the main hypothesis of the work, revealing stronger evidence of criminogenic effects in counties where meth labs were likely in operation before the enactment of the policy. However, they do not exclude the possibility that similar criminogenic effects could also arise in counties where no such labs were found.

Second, Table A8 of the Online Appendix shows that counties in which at least one lab was found were more populated and were experiencing more crime than other counties in 2003, the year before the first OTC restriction was implemented. Crime responses in ‘Meth producer’ counties could be explained by prolific criminal networks gravitating around the domestic production of the drug, which might have been more reactive to the implementation of the laws. Also, property crimes might have increased more in more populated counties—where meth labs were found—because more targets were available for criminals in these locations. In other words, other counties’ characteristics may explain the stronger evidence of criminogenic effects detected in ‘Meth producer’ counties after the passage of the laws. Admittedly, disentangling the exact mechanisms behind the increase in crime in ‘Meth producer’ counties is difficult and goes beyond the scope of my paper. However, the dynamics described are consistent with the main hypothesis of the work, which suggests that the implementation of the OTC restrictions led to an increase in crime via the disruption of the domestic market for methamphetamine that was plausibly more prevalent in ‘Meth producer’ counties.

*Further robustness checks* In Table 6, I show other robustness checks that examine the sensitivity of  $\beta_4$  estimated on ‘Meth producer’ counties in the entire sample of analysis.<sup>25</sup>

Column (1) of Table 6 shows the linear specification with crime rates per 100,000 people as its outcome. Column (2) reports the results of a Poisson count data model. Column (3) shows the log model as in the baseline specification, for comparison purposes. Column (4) adds state quadratic trends to column (3). Column (5) adds state cubic trends to column (4). Column (6) adds 721 county linear trends to column (5). Column (7) excludes the month in which the OTC restrictions are coded as a fraction of the month when the law was implemented. Column (8) uses a balanced panel of 23 months before and after the month in which the regulation was implemented. Positive and significant estimates across the different specifications behave similarly to the state-level analysis presented in Table 3.<sup>26</sup>

### *Arrests, length of the effects, and discussion*

*Changes in policing effectiveness* After the passage of the reforms, local enforcement agencies might have started chasing local methamphetamine producers. This could have led police officers to be less effective in deterring property and violent crimes, potentially inflating the criminogenic effects of the laws. To explore this possibility, I examine the effects of the reforms using estimating equation (1). I focus on drug-related arrests (sale/manufacture and consumption of truly addictive synthetic narcotics and other dangerous non-narcotics) and arrests for assault, burglary and larceny.<sup>27</sup> I report the results in Table A10 of the Online Appendix for the whole estimating sample, and in Table A11 of the Online Appendix for the sample excluding states where only the federal CMEA was adopted.



TABLE 6  
ROBUSTNESS CHECKS: COUNTY-LEVEL ANALYSIS

	Linear model (1)	Poisson count data (2)	Log model (1) (3)	+ State quadratic trends (4)	+ State cubic trends (5)	+ County trends (6)	No implementation month (7)	Balanced panel (8)
<i>Panel A: Larceny</i>								
OTC	9.443***	0.0330***	0.0393***	0.0346**	0.0317**	0.0318**	0.0392***	0.0409**
restriction	(3.474)	(0.0109)	(0.0150)	(0.0143)	(0.0146)	(0.0146)	(0.0151)	(0.0178)
<i>Panel B: Burglary</i>								
OTC	2.567**	0.0249**	0.0332**	0.0218*	0.0258**	0.0258**	0.0310**	0.0330*
restriction	(1.054)	(0.0102)	(0.0137)	(0.0124)	(0.0129)	(0.0128)	(0.0141)	(0.0170)
<i>Panel C: Assault</i>								
OTC	5.068***	0.0252***	0.0400***	0.0405***	0.0350***	0.0351***	0.0397***	0.0358***
restriction	(1.557)	(0.00975)	(0.0100)	(0.00925)	(0.00921)	(0.00920)	(0.0109)	(0.00951)
Observations	54,075	54,075	54,075	54,075	54,075	54,075	53,354	33,887
Counties	721	721	721	721	721	721	721	721

Notes

This table shows robustness checks for the  $\beta_4$  coefficients obtained using estimating equation (3). I use the sample including 47 states, and focus on the subset of counties with at least one meth lab seized in 2004. Column (1) shows the linear specification with crime rates per 100,000 people as its outcome. Column (2) shows a Poisson count data model. Column (3) shows the log model as in Table 5. Column (4) adds state quadratic trends to column (3). Column (5) adds state cubic trends to column (4). Column (6) adds county linear trends to column (5). Column (7) excludes the month in which the OTC restriction is coded as a fraction of the month when the law was implemented (54,075 – 721 = 53,354). Column (8) uses a balanced panel of 23 months before and after the month in which the regulation was implemented, so that each county is observed an exact number of times (721 counties \* 47 time periods = 33,887 observations). Crime is transformed into logarithmic form  $\log(1 + z)$ , where  $z$  is the crime rate per 100,000 people, unless otherwise specified. Regressions are weighted by population of the agencies reporting crime in a county-month (but not in the Poisson model, as the likelihood function fails to converge when state-specific trends are included). Standard errors clustered at the state level are shown in parentheses. \*\*\*, \*\*, \* indicate significance at the 1%, 5%, 10% level, respectively.

As in Dobkin *et al.* (2014), I do not detect any change in arrests for drug possession or sale of narcotics and non-narcotics. This suggests that enforcement officers did not target drug users and distributors after the passage of the reforms. The analysis reveals instead a proportional increase in arrests for assault and burglary. Estimates are around 4.5% ( $p < 0.01$ ) for assault and 3.4% ( $p < 0.05$ ) for burglary. For larceny, the point estimate is around 1.5% but is not significantly different from zero. Excluding states where only the federal CMEA was implemented, I detect similar effects for assault and burglary. Moreover, the point estimate for larceny is 3.4% with  $p < 0.05$ . These results reduce the concerns that the criminogenic effects of the laws may be inflated by enforcement agencies redirecting resources to eradicate the domestic markets for methamphetamine. If that were the case, then we would have expected to observe an increase in drug-related arrests, and a decrease or no effects in arrests for property and violent crimes.<sup>28</sup>

*Length of the effects and possible channels* Crime data do not include the necessary details needed to pin down the exact mechanisms behind the emerging criminal activity and the dynamics of the effects. However, the evidence gathered in my analysis allows for an informed discussion of the possible mechanisms at play in this context. The increase in arrests, proportional to the related increase in crime, could help to explain the short-term effects of the laws. Local enforcement agencies responded to the increase in property and violent crimes by executing more arrests, hence incapacitating criminals behind bars. This might have prevented them from committing crimes over a longer time horizon. Also, the length of the effects might be consistent with possible motivations behind the crime increase, such as the initial attempt of domestic producers to keep the production ongoing, committing thefts to compensate for the higher costs of production imposed by the laws, before finally quitting the market, and a momentous increase in the cost of addiction generated by the higher search costs for the drug.<sup>29</sup> Similarly, the effects on violent crimes might reflect the initial competition among criminal groups to fill the vacant demand for drugs and to fight over the newly available illicit rents.<sup>30</sup>

*Discussion* Dobkin and Nicosia (2009) examine a different supply-side measure targeting methamphetamine in 1995 in California. This was one of the largest shocks that occurred in any illegal drug market in the USA because of the substantial concentration in the supply of methamphetamine precursors during the 1990s. They found a large reduction in supply and demand for drugs, but no significant impact on property and violent crimes. Although, admittedly, reconciling the differences in the two studies is difficult because the two policies were different, affected different locations, and were implemented with a time lag of more than 10 years, a key difference appears to be related to the effectiveness of the laws in disrupting the supply and demand of the respective drug market. The policy that I examine had only a limited effect on supply because it affected small domestic producers constituting a small share of the methamphetamine market (without disrupting larger producers or Mexican cartels)—and it had no effect on demand. Thus it could be that these interventions incentivized violence due to the emergence of competition between multiple unaffected actors, as well as thefts committed to keep usage constant. In other words, my findings suggest that policy interventions that have a rather partial effect on supply and no impact on demand could open up the way to unwarranted crime responses. Overall, I believe that these heterogeneous effects highlight the importance of producing more research aimed at increasing our understanding of the possible impacts of supply-side measures on criminal activity.

#### IV. CONCLUDING REMARKS

The US government employs supply-side interventions as a leading strategy to counteract the expansion of dangerous illegal drug markets. Deepening our understanding of the criminal consequences of these interventions is therefore critical for the design of cost-effective regulatory frameworks: these should be aimed at constraining the growth of illegal drug markets while also mitigating the risks associated with possible unintended criminal responses.

This paper examines the effects on crime of regulations that reduced access to common medicines containing pseudoephedrine, which are key precursors needed to manufacture crystal methamphetamine in small-scale operations. These policies reduced the number of operating labs by around 36%, heavily disrupting the domestic market for methamphetamine. I find that the intervention led to an increase of around 4% in both property and violent crimes. The effects are concentrated within 7 months after the implementation of the laws, and are stronger in counties where labs producing methamphetamine were previously in operation. My findings are consistent with the notion that the criminogenic effects are caused by the disruption of the local domestic markets for methamphetamine.

Despite the passage of the reforms, traffickers have found ways to avoid these restrictions, and the number of domestic meth labs in the USA has been increasing recently.<sup>31</sup> This motivates an ongoing policy debate centred on the classification of pseudoephedrine-based medications as prescription-only drugs. Oregon (as of July 2006) and Mississippi (as of July 2010) are the only two states that have classified pseudoephedrine and ephedrine as Schedule III substances, requiring patients to obtain a medical prescription to get these medicines. Between 2010 and January 2013, at least 69 similar bills were introduced in 18 US states. Most of these bills were referred to specific committees, and none of them has been approved yet.

My findings convey timely policy implications. They suggest that further restricting access to methamphetamine's chemical precursors could lead to an increase in criminal activity in the USA, particularly in areas where the domestic production of this drug is more prevalent. Thus my results provide direct support for supply-side interventions that should be implemented in conjunction with crime-prevention strategies targeting well-known hotbeds for the domestic production of methamphetamine. Also, my work suggests the need to implement supply-side interventions that internalize the probable impacts on consumers. For instance, parallel social programmes aimed at managing the demand side of the market might attenuate the emergence of criminal responses that could be connected with the compulsive need of heavy drug users to fund their addiction. This suggestion is in line with the recently signed 21st Century Cures Act, which will provide \$1 billion in funding for demand-side interventions, such as prevention and substance abuse treatment, which aim to reduce the prevalence of addiction (Alpert *et al.* 2018).

My paper also informs policymakers outside the USA about possible drawbacks of similar regulations to the ones examined in my work. In fact, Eastern Europe, Africa, South-east Asia, the Middle East and Australia are experiencing a giant rise in the crystal meth epidemic and facing the negative consequences associated with the propagation of this dangerous drug (UNODC 2015). To conclude, I believe that interesting and promising avenues for future research will emerge in the attempt to deeply scrutinize the interplay between government measures that legitimately challenge the expansion of illegal drug markets, with possible criminal responses originating from changes in the supply and demand sides of these widespread clandestine markets.

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## NOTES

1. The void in the domestic supply was filled by methamphetamine arriving from Mexican cartels. The share of methamphetamine produced in labs located in Mexico increased from 65% to 80% after the laws were implemented (see [https://www.ncjrs.gov/ondcppubs/publications/pdf/interim\\_rpt.pdf](https://www.ncjrs.gov/ondcppubs/publications/pdf/interim_rpt.pdf), accessed 30 May 2020).
2. See the final section for a more detailed discussion about these aspects.
3. A typical domestic producer of methamphetamine is a heavy user who started manufacturing the drug to sustain his drug addiction (Eck and Gersh 2000). The laws might have increased the cost of addiction for these users, who were previously able to sustain the habit by inexpensively producing methamphetamine in their small labs, and after the laws had to pay more in order to keep usage constant. Also, users who were not producing methamphetamine but were gravitating around domestic production might have experienced an increase in the search costs for the drug, which before the laws was readily available within the close network of drug addicts (Sexton *et al.* 2006).
4. The National Association of Counties administered an annual survey to local enforcement agencies to assess the harms connected with the proliferation of various illegal drugs. In the 2005 report, 58% of the counties ranked methamphetamine as the most dangerous drug within their territory because of its ability to foster property and violent crimes. In 50% of the counties, 1 in 5 jail inmates was housed because of meth-related crimes. In the other half of the counties surveyed, 17% reported that more than half of their prison population was incarcerated because of meth-related crimes.
5. This estimate was obtained by considering: the costs of crime for larceny (\$2139), burglary (\$13,096) and assault (\$87,238) reported by Heaton (2010); the pre-intervention monthly crime levels in counties where labs were operating prior to the reform; and the length of the impacts on crime detected in my analysis.
6. For a systematic review of the effects of drug law enforcement on drug market violence see Werb *et al.* (2011). See also Reuter (2009).
7. Cold tablets are mixed with sodium hydroxide, anhydrous ammonia, iodine, matches containing red phosphorus, Drano, ether, brake and lighter fluid, and hydrochloric acid. These products are easily found in local stores. The entire chemical process is performed in self-made chemical labs hidden in apartments, caravans, garages or hotel rooms (see Figure A1 of the Online Appendix).
8. I did not find any evidence suggesting that there was a concern that the OTC restrictions would have affected property and violent crimes, or that these restrictions were implemented alongside monitoring or crime-prevention programmes. If law enforcement agencies were concerned about the impact on property and violent crimes, then it is plausible to assume that more resources would have been directed toward preventing this potential increase in crime. In this case, my estimates of the criminogenic effects of the laws could represent a lower bound of the true effects of the reforms.
9. The estimation of heterogeneous effects by type of law could help us to understand which restrictions had the largest impact on crime. Unfortunately, the laws were typically implemented in bundles, preventing a credible identification of the separate effects of each restriction.
10. The use of data voluntarily reported cannot inform us about possible criminogenic effects arising in non-reporting agencies. If the decision of not reporting property and violent crime is uncorrelated with the presence of methamphetamine in the territory (an assumption that seems plausible but that, however, cannot be formally tested due to the absence of data on the spread of methamphetamine at the agency level), then my results could be interpreted as an average effect of the policy across all US enforcement agencies.
11. The Online Data Appendix provides detailed information on the construction of the final databases in which agency data are aggregated at the state and county levels.
12. These data were downloaded from <https://www.dea.gov/clan-lab> (accessed 1 June 2020).
13. The same controls were also used by Dobkin *et al.* (2014).
14. Suppose that a state enacted a law on 21 April 2006. I would record the variable  $OTC_{st}$  as 0 for all the months prior to the implementation, 1 for all the months afterwards, and  $(31-21)/21 = 0.476$  for the month of April 2006.
15. It is not feasible to include a full set of event dummies. This inclusion would be collinear with state fixed effects and time fixed effects.
16. County-specific monthly covariates on weather conditions and socioeconomic factors are not available.
17. Larceny is the unlawful taking, carrying, leading or riding away of property from the possession of another. This is the most common type of theft crime. Common types of larcenies include shoplifting, pickpocketing, purse snatching and theft of objects from motor vehicles. Burglary is the unlawful entry into a structure to commit a felony or theft.

18. Connecticut, DC, Maryland, Massachusetts, Nevada, New Hampshire, New York, Pennsylvania, and Utah were subject to only the federal CMEA. DEA data indicate that in states adopting an internal law, 4.2 labs per 100,000 people were discovered in 2004. In contrast, in jurisdictions that were subject to only the federal law, just 0.24 labs per 100,000 people were detected in the same year. The meth epidemic was stronger in the central and western parts of the USA.
19. It is not possible to consistently estimate the effects of the federal CMEA in the subset of states that were subject to only this law. This is because the date of implementation (April 2006) is the same across this subset of states, and the rollout dummy ( $OTC_{s,t}$ ) would be perfectly collinear with the common time trends indicator ( $\gamma_t$ ).
20. Information on community programmes for treating addiction in the USA at the state-month level during the period of analysis is unavailable to the researcher.
21. These are the crimes that appear more frequently in the data. The outcome means for larceny, burglary and assault are 210.7, 67 and 122.7 crimes per 100,000 people, respectively. See Table 1.
22. Keeping the within-state variation fixed and moving the sample years before and after the implementation of the laws allows us to examine whether the results are driven by seasonality.
23. Figure A2 of the Online Appendix shows the event-study analysis excluding state-specific linear trends. Results are similar.
24. The ideal data to test this hypothesis would be a census of meth labs in the USA before the laws were effectively implemented. Because of the criminal nature of this business, such records do not exist. Instead, I used information on the locations of labs discovered by law enforcement agents prior to the intervention. To the extent of my knowledge, no national county-level data exist on the usage of methamphetamine before the laws went into effect, which could also have been used as an indirect proxy for the local domestic production of the drug.
25. As for equation (1), I also show the sensitivity of the  $\beta_4$  estimates by gradually including state-specific linear trends and all the covariates in the 'Meth producer' and 'Other' counties (Tables A6 and A7 of the Online Appendix, respectively).
26. Dobkin *et al.* (2014) detect weaker impacts of the laws on the domestic markets for methamphetamine in counties sharing the borders with states with no regulation in place, possibly because local producers were able to circumvent the laws obtaining pseudoephedrine-based medications from these neighbouring states. I hence examine the impact of the regulations on crime in counties bordering a state without similar restrictions. All details are provided in the Online Appendix.
27. The Online Data Appendix discusses the details of the UCR data on arrests. Table A9 of the Online Appendix provides descriptive statistics. Arrests for sale or possession of methamphetamine are not separately reported and are included in the category of 'Other dangerous non-narcotics'.
28. Also, the implementation of the policies made it easier for local enforcement officers to detect the few labs still in operation. This is because the laws required the names of customers to be noted in special logbooks that were accessible to police forces. If anything, this seems to reduce the concerns that an increase in policing effort aimed at contracting the local production of methamphetamine could drive the rise in crime detected after the passage of the reforms.
29. Sexton *et al.* (2006) describe that increased difficulty in finding methamphetamine precursors, and their higher prices on the black market, led the majority of domestic producers to eventually leave the illicit market. In affected communities, there was some delay in the arrival of Mexican methamphetamine, and users had to 'hunt' down methamphetamine. Also, in some cases, the Mexican methamphetamine was more expensive than that produced domestically.
30. Dobkin *et al.* (2014) document a large drop in the purity of meth at the beginning of 2006, while many of the OTC regulations happened midway through 2005. I have estimated event-study specifications comparing early vs. late adopters in an attempt to assess whether the short-term nature of the crime increase is driven by this shift in the market. This analysis leads to imprecise estimates that are available on request.
31. According to the 2018 National Drug Threat Assessment, methamphetamine is available throughout the USA, with the highest availability in the West and Midwest regions of the country. However, in recent years, methamphetamine has been increasing in prevalence in areas that have, historically, not been major markets for the drug, particularly the North-east.

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**SUPPORTING INFORMATION**

Additional Supporting Information may be found in the online version of this article:

**Appendix A:** Tables A1–A11; Figures A1 and A2

**Appendix B:** Graphical spillover analysis; Table B1